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LECTURES ON GENERAL THERAPEUTICS.

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LECTURE I.

GENTLEMEN.—The preceding lectures of this course have been devoted to a consideration of the causes from which diseases proceed; to a description of the symptoms by which they are indicated, and of the elementary affections by which they are constituted; and, finally, to an exposition of the method according to which these materials are to be analyzed so as to make up, what are called in medical language, the diagnosis and prognosis. These are in themselves interesting objects of study, but their real importance consists in the relation they bear to the study of Therapeutics, or the treatment of disease. To this they are the necessary preliminaries.

The original notion among mankind probably was, that each disease is a thing by itself, a distinct individual entity; that one disease differs from another, just as one plant differs from another, or one animal from another;—a peach from a plum; a horse from a dog. In conformity with this notion of disease, was that of the nature of remedies;—that each disease, having its own peculiar character, had also its own peculiar remedy; that this medicine was good for one disease, and this for another; this for gout—this for fever—this for cough. The same notion enters even now into the popular idea of therapeutics, and you may have yourselves entered the profession with some vague conception of the same kind. Hence have arisen systems of nosology. These arranged disease into classes and orders; genera and species; just like the subjects of Natural History. These attempts at classification undoubtedly had their use; but you are aware that, in the present state of medical knowledge, they are discarded as superfluous; just as we take down the scaffolding of a building after its erection has made a certain progress.

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I formerly endeavored to explain how it was, that, whilst diseases are many, the elementary affections on which they depend, are few; that diseases vary from one another, not by each being possessed of an exclusive individual character, but, by the fact, that the same elementary affection produces what we call a separate disease, according to the organ or texture in which it is seated, the function which it interrupts, or the state of the constitution in which it occurs. Now something like this is true of the treatment of disease. Diseases are many, but the principles on which we proceed in their treatment are few. The most important parts of treatment relate to disease in general and not to particular diseases. Before, then, proceeding to the details of individual diseases and their treatment, it is proper to explain the principles of General Therapeutics.

It is a matter of common observation that, of some diseases prevailing extensively—such as common catarrh—all, or nearly all persons recover; and this whether they use remedies or not: that of other diseases, as typhoid fever or pneumonia—under the same circumstances—the larger number also recover, but a certain proportion die; whilst of other diseases—as plague and cholera—though a few recover, nearly all die, whether they have been the subjects of medical treatment or not. Now why this different result? Why and how do persons get well who use no remedies; and if a certain proportion get well, why do not all? This is an inquiry that should be preliminary to all questions of treatment.

This inquiry is twofold, and our first purpose is to learn why and how do patients recover who use no remedies.

It is obvious that, in order to this, there must exist in the system some power which contends with disease, seeks to remove it, and in these cases does so successfully. The recognition of such a power in some form or degree, among the careful observers of injury and disease, is probably as old as our art; but the nature, the mode and extent of its operation, have not always been as clearly perceived as they now are. It was early designated as the "*vis medicatrix naturæ*"—the curative force of nature; an expression less employed than formerly, but worthy of being remembered as the original form of words denotive of the idea. It is now referred to under other and various names and designations, as the sanative effort of the system, the restorative tendency of Nature, &c., but all refer to the same principle.

This is not, as so distinct a mode of expression might seem to imply, a separate or independent principle, coming into play when an injury has been inflicted or a disease induced, and, after recovery, suspending its activity and lying dormant till a new occasion calls for its beneficent operation. On the contrary, it is always and everywhere present, and always and everywhere in exercise. It is, in fact, only a modified activity of that power which constantly maintains the body in a state of healthy organization and

action. This power watches over the condition of every part; keeps up its relation with every other part; presides over its nutrition; repairs its waste; heals its injuries; remedies its diseases. The purpose, the mode, the degree of its activity, are determined by the condition of each part, and by the condition of the system as an aggregate of parts. It accommodates itself to the variations of this condition; so that, while in health, it is merely concerned in maintaining each part in its integrity by the healthy processes of nutrition, in disease it is concerned in removing that in which disease consists, by what are called the processes of disease. Its office, in health, is to keep the parts in a normal condition. Its office, in disease, is to bring them back to a normal condition. The processes of disease are only the processes of health modified and adapted to a peculiar exigency. The instruments are the same; the materials are the same. Inflammation is a modified form of nutrition; spasm, of muscular contraction; fever is a modified result of the same law by whose influence increased vascular action is induced by the increased activity of any organ. The processes are simply varied from their ordinary character, because the purpose for which they are established is different from that to which they are ordinarily directed. The quality of life is such, that it is capable of using the same means for different purposes—for formation, for nutrition, for growth, for repair, for restoration—just as a skilful workman employs the same tools and the same materials in the repair of a machine, that he has already employed in its construction.

This principle is universal in organized matter, animal and vegetable, but it is exhibited in different degrees, and with various limitations. It is more restricted in man than in the animals below him. In many of them, it not only keeps good the organization of parts and repairs partial injuries; it may even reconstruct organs that have been removed or destroyed—as the claw of a lobster or the eye of a newt—but in man it is only capable, in health, of maintaining the organs in a normal condition, and, in disease or injury, of bringing them back to that condition.

But the efforts of this principle, as we have already seen, are not always successful, and this brings us to the second branch of our inquiry:—Why are they not always successful? Why are they so often unavailing? Why are they so often defeated? We shall have occasion to consider this more at large hereafter. It will be sufficient to say now, that the causes are many and various; such as, among others, the nature and severity of the disease or injury; the state of the patient's constitution; the character and functions of the part affected; the favorable or unfavorable conditions under which the patient is placed; his injudicious management on the part of those about him. In disease, there is always a contest between two antagonistic forces—the force of disease, tending to destruction; and the force of recovery, tending to prevent destruc-

tion. The result will depend upon the relative strength of the contending parties. But whatever be the force of the disease, we are always able to detect indications of the effort to remove it. Even in those maladies which are almost necessarily mortal, as cancer, consumption, tetanus, or hydrophobia, we trace the constant operation of the same restorative principle in the character of many of the processes that are going on, and in partial periods of relief and suspension of progress.

But it may now be asked, is there no other dependence but upon this principle of spontaneous recovery? Has art no direct resources? Are there no absolute remedies by which disease can be controlled and expelled? It always has been and still is believed that there are. The simple and primitive idea of treatment, to which I have already referred, is founded on this belief. It has been difficult to determine how far this belief is well founded, because very generally in the practice of medicine care has not been taken to distinguish between the direct influence of remedies and the results of the sanative principle; between what is due to Art and what to Nature. Hence, as some remedy or other has usually been given, the recovery is apt to be ascribed to its influence. It is not till the treatment of disease is carried on with a clear and distinct appreciation of these two separate principles, and with constant reference to the part which each severally takes in bringing about the result, that we can properly determine their respective value, and thus learn how far we are in possession of remedies of a direct and positive character.

Still, the belief in such remedies exists among physicians, and is the result of a long course of observation and experience; but there is a wide difference of opinion with regard to their number, their amount of efficacy and their mode of operation. This direct remedial agency, so far as it exists, appears to be founded upon the relation which is maintained between the human system and other substances in nature. Probably every such substance bears a peculiar relation to this system, in consequence of which it produces peculiar effects upon it. In the case of powerful agents this is clear enough, as in those having a strong smell or a strong taste, or acting as medicines and poisons. But there are considerations which tend to show that even substances that appear inert, in the ordinary mode of application to ordinary constitutions, are capable of producing decided effects when differently applied, or applied to peculiar constitutions. Thus, prussic acid, as combined in the common peach, produces no sensible effects, but when exhibited in a concentrated form may speedily destroy life; whilst, on the other hand, the most virulent poisons, as strychnine, in a small dose and largely diluted, may be taken without injury. Still further, some substances from which most persons perceive no peculiar effect, act upon certain individuals almost as poisons, such as mutton, milk, cheese, honey, &c. An eruption upon the skin is produced in cer-

tain persons by the contact of rose leaves, and a species of asthma, or difficult breathing, by emanations from them, from hay, from ipecacuanha, and probably various other vegetable substances unknown to us, although their presence is not obvious to the senses. Emanations from the common domestic cat give rise, in some constitutions, to faintness, nausea and vomiting, and this even where the animal is not known to be present, and its odor has not been perceived. Such susceptibility, it is true, is an individual peculiarity, called in medical language an *idiosyncrasy*. Probably, however, it is only the exaggeration of a relation common to all mankind, but which in others requires the application in greater quantity or of greater intensity. So that very likely there is no substance that in some quantity or in some degree of intensity is not capable of producing a cognizable influence upon the human system.\*

At any rate, whether this be so or not, it is upon such a relation that those articles which have been selected as medicines depend for the effects they produce. They may be divided, according to the manner in which they operate upon disease, into two classes: the *first* contains those that are directly remedial; the *second* contains those that are indirectly remedial.

I. The articles of the first class are supposed to act directly for the removal of disease—they are primarily curative. Thus Peruvian bark, quinine and arsenic, have a direct influence on certain diseases characterized by distinct paroxysms and intermissions—such as intermittent fever, hemicrania, intermittent catarrh and neuralgia, and some others having this same element of distinct intermission in common with them. Of the same direct nature is the influence of mercury and iodine upon syphilis—of colchicum upon gout and rheumatism—of iron in anaemia—of lemon juice in scurvy—of ergot upon the parturient uterus.

I mention these as the most distinct cases of an absolute power on the part of drugs to remove disease, and probably few physicians would doubt that they have this power. But the possession of the same sort of power has been claimed, from time to time, for a great many other articles, about which there has been and is a variety of opinion; and it is to be remarked, that as disease has been observed more closely, and its course judged of in the light of more advanced science, the number of these articles has steadily diminished. Even of those medicines with respect to whose influence there is no reasonable doubt, it is not intended to imply that this influence is always exerted—that they are infallible remedies. Far from it. On the contrary, they very often fail in the cases to which they are appropriate. But so far as they have any effect, it is directly to remove that condition in which the disease consists, and their power depends upon a peculiar relation

\* Some remarkable facts illustrating these statements may be found in the history of Casper Hauser.

between the remedy and that condition. In other words, they are specific remedies. Still, even where we depend upon specific remedies, it does by no means follow that the whole work of recovery is performed by them. It is always necessary that a part at least of this work, in order to its completeness, be performed by nature. Suppose that quinine has extinguished the paroxysms of an intermittent. There is usually left behind an impaired state of the functions which it does not remedy, but which nature gradually restores. Mercury destroys the specific character of a syphilitic ulcer, but the ulcer is only healed by a spontaneous process. The removal of disease in this way bears a sort of analogy to a surgical operation, in which the knife removes the diseased part, and the wound left behind is healed by the powers of nature.

This, so far as we have the means of following it, is the most simple, perfect and satisfactory method of treating disease. Its purpose is simple and distinct. It implies the knowledge of a distinct object to be effected, and of a distinct agent with which to effect it. Unhappily, our knowledge of disease itself is so limited, and our knowledge of the exact power of remedies is also so limited, that it is capable of but a narrow application. Yet such is its simplicity and directness, that it has a great charm to many minds, even among physicians. So conformable is it, also, to popular comprehension, and to popular notions of disease, and even to those of many medical men, that there is a constant tendency to extend its application and to believe in its capacity for extension. If we examine the medical journals of the day which give an abstract of the various new propositions for treatment, it is found that no inconsiderable proportion of them are based upon this relation of remedies to disease.

Upon the same principle is founded—if I understand it aright—that part of the theory of homeopathic practice which relates to the efficacy of its medicines. It teaches that for every morbid condition there exists a specific remedy—a distinct antidote. No method of treatment could be more perfect in theory or more satisfactory in practice, if it were actually founded in truth. But there is a two-fold difficulty in the way of accepting it as a sufficient system. First, there is a want of evidence that such antidotes have been discovered, or that they even exist; and second, even admitting their existence, there is a great anterior improbability of their being capable of any positive effect upon the human system in the inconceivable state of dilution in which they are exhibited, whilst there is no sufficient body of proof to balance this improbability.

These remarks relate to the powers possessed by remedies for the direct removal of disease; but there is another view of remedies, somewhat of the same kind, which is of great importance. There are few, as has been stated, having a direct power over disease, but there are a great many having a direct power over

symptoms. It is often desirable to palliate a symptom, when we have no power over the essential malady. If opium cannot cure the disease, it will at least remove the pain which the disease produces. If antimony cannot cure pneumonia, it will at least diminish the febrile activity accompanying it. Considered in this point of view, medicines become most important as subsidiary agents, when the character of a disease obliges us to leave it to run its natural course.

II. The articles of the second class, when they are efficacious at all, are efficacious in a different way. They have a distinct operation—often more distinct than that of the preceding class—but an operation not primarily remedial, though it may be so secondarily. Of this kind are emetics, cathartics, narcotics, strychnia, digitalis, blisters, and many others. They do not directly remove disease, but they produce definite changes in the condition, action, and perhaps structure of organs, the indirect result of which may be its removal. The relation of remedies of the first class is directly with the disease. The relation of those of the second class is directly with some organ or function which may or may not be involved, and only indirectly, with the disease. Quinine subdues intermittent fever by its direct operation. Emetics and cathartics, if relied upon for the same purpose, can only bring about the same result by the vomiting and purging they excite, and this may indirectly arrest the course of the disease. We know that strychnia will produce contractions in the muscles of a palsied limb, but we do not know that it will, as a consequence, restore its natural power of motion. We know that digitalis will diminish the frequency of the pulse, but not that it will cure the disease which has produced it. We know that cantharides will blister the skin, but not that the blister will relieve the inflammation for which it has been applied. The effect in the first class is curative; in the second class, physiological. The principle upon which the agents of these classes act, so far as they are beneficial at all, is thus entirely different in the two. Their value as remedies is to be judged of from a different point of view, and by a different kind of evidence.

A great number—perhaps the greater number—perhaps all medicinal agents—appear to bear a special relation to particular organs, and to produce their effects through those organs. Antimony, however, introduced into the stomach, the veins, the rectum, or applied to the skin, acts upon the stomach, cantharides upon the bladder, opium upon the brain and nerves, strychnine upon the muscles, cathartics upon the bowels, mercury upon the salivary glands, phosphorus upon the bones of the face. This list might be extended, but it is sufficient to suggest the fact as of some importance in studying the effects of remedies.

Diseases may be treated, then, according to two distinct methods:—

1. By leaving them mainly to the influence of the sanative principle, or the *vis medicatrix naturæ*.

2. By the employment of absolute remedies that we believe to be possessed of the power of removing that condition in which disease consists, either directly or indirectly.

Whatever be the relative value of these two methods, of which different opinions may be entertained, there can, I think, be no doubt that under one or the other are comprised all the agencies that are ever concerned in the treatment of disease. Consequently, if all treatment resolves itself into these two methods, all treatment should be studied in relation to them, and it is only by keeping them continually in view, and determining, as far as practicable, upon which depends the efficacy of any course we adopt, that we can arrive at any clear and distinct views of the results of our practice. Now there is really no such incompatibility between these two methods as renders it inconsistent to rely upon both in the same case or the same diseases. On the contrary, the best practice is probably that which combines them. Thus, where the main reliance is upon nature, there are few cases in which, at some period in their course, some absolute remedy may not be employed either to directly aid in supporting the system through the disease, or else to remove some obstacle or relieve some symptom which interferes with its successful progress. There are few plans of treatment, either in systems or in the practice of individuals, where these two methods are not combined. There has always been a certain undefined reliance on the powers of nature, whilst, at the same time, a great variety of distinct remedies have been employed with an equally undefined conception of the manner in which they are to prove useful, whether by their own efficacy, or by indirectly promoting the natural recovery. In order to clear and philosophical views of practice, it is necessary that the physician should first judge what amount of benefit he is to expect from the efforts of nature, and then, if he uses remedies, whether he merely uses them in aid of these efforts, or whether he expects from them a distinct effect independent of these efforts. I acknowledge that these are points very difficult to determine, but the more nearly we approach to their determination, and the more constantly we attempt it, the more will the results of our experience become definite and available; without the attempt, we can never tell, on recovery from disease, to what we are to attribute recovery—whether to nature or art; and if wholly or partly to art, what has been the particular agency of our different measures in the result. Without it, the largest experience may present only a dreary waste of vague inferences, and of loosely-observed and unavailable facts.

It is only by keeping an eye on this principle of judgment that we can attain to an understanding of the exact limits of our art—to a knowledge of what can and what cannot be done. In any

practical department, it is an indispensable condition of success to determine the limit between the practicable and the impracticable. What should we expect of the mechanic who attempted to construct a machine upon the principle of perpetual motion? Must it not vitiate all his calculations with regard to its working? He does not understand the limits of his art. He aims at the impracticable. The physician may do the same thing. He may aim at the impracticable for want of a proper appreciation of the limit of his power over disease. Suppose he were to attempt to arrest measles, or smallpox, or scarlatina at its onset, and prevent them from going through their regular course? Would not this be an attempt as preposterous as that to work a machine by perpetual motion? Now our present knowledge does not enable us to determine, in all cases, what the limit is, but it is no extravagance to say that any deliberate consideration of the matter at all is apt to be overlooked. To what heroic treatment have patients been made to submit; to what torments have they been subjected; what quantities of drugs have they been made to swallow—not only to effect purposes to which they were incompetent, but purposes which were in their very nature unattainable. We are constantly disappointed in the result of our plans of treatment, and in the effect of our medicines. Is it not often because we have aimed at controlling a state of disease which is only part of a necessary course of processes, or at the removal of a disease which is, in its nature, irremovable? In consequence of overlooking these considerations, it constantly happens that favorable results are attributed to remedies, with which remedies have had nothing to do; and, on the other hand, it constantly happens that unfavorable results are attributed to the disease, when they may in fact be owing to the remedies.

[To be continued.]

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#### THE VALUE AND THE FALLACY OF STATISTICS IN THE OBSER- VATION OF DISEASE.

A BOYLSTON PRIZE ESSAY, BY DAVID W. CHEEVER, M.D.

[Continued from page 483.]

THE numerical method affords us a numerical estimate of probability in a given number of cases; but this is not of much help to the practitioner at the bed-side, who has to determine the probabilities of the individual case before him, which may or *may not* be more or less similar to the cases estimated numerically. Perception, comparison and deduction are necessary for each individual, as well as in formulæ and tables. We may often learn from statistics, indeed, that of many effects or consequences, as the duration, course or result of a disease, one will occur more frequently under given circumstances than another. All this, however, admits of

no direct application to an individual case. So many special circumstances and influences come into play in each case, and complicate the question and calculation to such an extent, that the conclusion cannot be a safe one. Each individual case has its own special result, which could not be calculated exactly from statistics. If we know, for instance, that of those attacked by cholera, fifty per cent. die, and an equal number recover, we cannot, on that account, assume for an individual cholera patient, that the probability of his dying is precisely as great as that of his recovery. Such probability must be greater or less than the average, according to his age, habits, personal vigor, or the severity of the attack. Nor, again, if we knew that the mortality of smallpox was just one in four, could we predict for any given case of this disease, that his chance was just one in four. It would be either above, or below that number.

There is still another important source of error existing in each individual case; allied to, but in some senses distinct from, idiosyncasy. This is the "constitution" of the patient, as it is called; by which is meant the sum of all the influences of locality, station, hygiene, occupation, habit, diet, or accident, which have acted upon the individual from the time of his birth, until the period of the disease we are treating. And not only this, but influences called hereditary, in some diseases of great gravity, which extend back through generations before his birth. It is plain that we can never expect to attain a knowledge of all these influences by the most detailed examination or the largest tabulations; yet such knowledge is indispensable to render our facts, or units of calculation, not identical, but *strictly comparable*. It is, after all, with much justice, that following the popular idea, we hear patients say, "I value my family physician because he understands my constitution": and so, to the extent permitted by his finite faculties, he does.

The positiveness of principles (and principles are only generalizations), says an empirical writer, applies only to the principles themselves, and not to the individual phenomena and relationships, by the aggregate of which they are composed. This applies to both pathology and therapeutics. The positiveness of the law cannot apply to single instances. So diagnosis, prognosis and treatment must ever depend mainly on an accurate knowledge of the individual cases themselves.

To avoid as many as possible of these errors, to compare justly the essential points of one of those great nosological groups of symptoms which constitute a single disease, how many observations, and how many separate comparisons must be necessary. Each symptom, essential or accidental; each sensation, positive or negative, present or wanting; every organ, as the skin, the tongue, the heart and lungs, as well as the more general functions indicated by the pulse, respiration and excretions, and, finally, *all the*

functions, whether morbidly affected or not, must have tables, sub-tables, columns and balances by themselves. So, too, must the previous history of the patient be detailed with a fidelity which would require a mind and memory far beyond the average of humanity, especially when sick; and the inquiry into so many minute symptoms demands a patience, an accuracy, and a power of differential diagnosis, such as very few of the best trained physicians possess. It would be impossible to find cases strictly comparable in all these respects. The presence of any one symptom cannot be offset by the absence of another; nor even in strictly identical symptoms can those either wanting, or in excess, be mutually balanced, unless by numbers extending over vast series of cases, and a very considerable length of time. It is plain that the busy practitioner can never find time to work out such numerical results for himself, but must depend upon the labor of others.

Not only does this independence apply to different classes of maladies, and to individual cases, but we shall find, by pushing our inquiries more into generalities, that no department of medical science follows from any other, but that each is distinct by itself. A knowledge of one branch of medicine cannot be deduced from any other, but each also must be studied by itself. Thus, our knowledge of anatomy is distinctly the result of pure observation. Physiology is not deducible from anatomy, any farther than we can trace a general adaptation of means to ends; as that the skull is made to contain, or the heart to circulate something, but what, its uses or functions, we know not.

Neither is pathology deducible from physiology, though the common opinion is just the reverse. We cannot infer the action of a function in disease, from its normal action in health; as, for instance, the excretion of urea by the kidneys in health, but its retention, and the excretion of albumen, a constituent of the blood, in Bright's disease. Physiology is useful to pathology only as a standard of comparison; and so it is very necessary, in judging of the comparability of many units and data of statistics. Etiology, too, does not follow from pathology, but is the result of observation alone. And finally, therapeutics are not founded so much on pathology, as on simple experience. All this must greatly increase the labor of the numerical method.

And since pathology is not founded on physiology, it follows that the action of medicines in disease cannot be safely inferred from their action in health. Therapeutics establish the relations between articles of the *materia medica* and certain morbid processes, not healthy ones. To a limited degree we have exceptions, as where an emetic substance would empty the stomach of a well, as quickly as of a sick person. But opium would often have far different effects in health or disease; pain alone being a perfect antidote to its poisonous action: and other instances might be cited. This does not interfere with the accuracy and value of the

researches of Lehmann, and Bidder and Schmidt, on the physiological action of water, coffee, alcohol, or mercury; but should be a caution to us not to infer too readily, that the same results will follow their employment in disease, as in health. We wish only to show that the numerical method, when carried into pathology and therapeutics, meets only with increasing uncertainty and labor, and can in no wise be aided by a knowledge of the more rudimentary branches of medical science, which may have been earlier acquired. Everything tends to prove that not only each person and each branch of medicine as a science or an art, has an individuality which cannot be readily compared to others: but that diseases, especially, admit of infinite variety in the degree, the number and the order of their symptoms.

Yet for the correct study of disease, an arrangement of symptoms into groups is important, on the one hand, and a searching into the intimate nature and causes of such groups, on the other. Otherwise we are in danger of erecting symptoms of many diseases into diseases themselves, while in fact they are only symptoms. And in popular speech we often commit this error, calling the most prominent symptom, as diarrhoea or haemorrhage, the disease. After we have successfully accomplished these nosological generalizations, we shall still find nature too complex for single observation, or numerical analysis. For our symptoms must be studied one by one, in the order of their development, and the precise period of appearance of each must be learned. One is apt to be confused by the multitude of symptoms which a patient presents. It is here that we discover the true value of hypothesis; not to analyze or theorize, but to enable us to isolate certain symptoms, previous to observation. Here, too, when it is practicable, experiment would come to our aid, because by it we can place certain symptoms in the best position for examination, thus judging of individual influences; and, in the physical sciences, we can repeat the experiments as often as we need. Fallacious grouping and the complexity of nature offer, therefore, serious obstacles to the correct analyses of the numerical method; while experiment is seldom admissible.

So many forces are at work modifying each other in life that correct induction is difficult, and the establishment of causal connection, at times, impossible. Vital phenomena move in curves, and not in straight lines: the non-appreciation of any one of the motive forces destroys the balance of antagonism, and would be fatal to the most careful calculations. Even the student of the physical sciences would find it difficult to ascertain the laws of motion, solely from bodies kept in a state of rest by opposing forces.

Again, the natural sequence of morbid phenomena is another source of error in establishing by numbers the natural history of disease. Not knowing the whole chain of causal connection, we are unable to decide whether one event takes place in consequence

of a stage of the disease arriving to determine it, or from some other cause, which we have under our control.

Thus, too, the very different influences under which patients may be placed, were they only so seemingly trivial as the temperature or the state of the atmosphere, have a modifying effect on each case, which we should seek in vain to reckon or account for. Time and place even may render the statistics of different epochs, or localities, wholly valueless for comparison with others. The season of the year, the tendency of the epidemic then prevalent, the very various effects of good or bad locality, in a hygienic point of view, may all concur to derange the true balance of calculation.

We are in danger also of forgetting that it is not alone the number, but the intensity also of the symptoms, which are to be the units of our statistics. But the latter cannot be accurately measured, nor expressed in numbers.

We have already adverted to the necessity of very large numbers in determining vital phenomena: the true use of the calculus of probabilities, and the approximative accuracy of which it is capable, we prefer speaking of under the head of the value of statistics, towards the close of this article.

We come now, then, to the crowning fallacy, among the *objective* ones, of the numerical method, namely, the influence of the Vital Principle.

Medicine can never be an exact science, since it deals with the vital principle—a principle in itself changeable, self-supporting, and self-regulating. Vital force is the great perturbative element which renders the results attained by the faithful student of Nature approximate rather than precise. The calm test of experiment and the pure logic of analysis in organic chemistry are rendered uncertain, through an imperfect knowledge of its laws. Chemical processes, which, duly carried out under similar circumstances, give always the same result in the laboratory, are often wholly and inexplicably changed in the living organism. It is very evident, then, how it must disturb the results of pathological statistics; nor does there seem to be much probability of its being finally fully understood.

Liebig, on the one hand, and a modern and very voluminous medical writer of this country, on the other, represent the two extreme views held regarding it. The former would reduce it to a physical, if partially unknown, phenomenon; the latter would exalt it into a position of complete supremacy over all merely physical laws. Both, probably, equally err. The second named gentleman has been betrayed by his enthusiasm into a denial of all chemical theories as applied to the human frame; of all discoveries of the microscope, and asserts, even, that the lacteal absorbents have open mouths. Liebig endeavors to bring all vital manifestations under a few universal laws of chemical affinity, and flushed by his

discovery of the interchange of oxygen and carbon in the capillaries, and its connection with animal heat, hastens to construct a theory of calorifacient and plastic foods, and to draw close lines of comparison between vitality and some forms of electricity; conclusions, to say the least, premature. To his views Carpenter inclines; but Paget and Dalton are disposed to view the vital principle as a higher law. Carpenter describes vital force as a power correlative with the physical forces; as, for instance, that it may be the mode of action of heat or electricity in the body; and that physical and vital forces mutually give rise to each other. Paget says that vital force is distinguished from all others by its powers of generating typical organic forms, or *modality*. Though correlated with the physical forces, it has no identity with them.

The distinction is clear between the force, which by chemical action prepares the material for constructing an organ, and the force which forms the ideal plan of the organ, and constructs it of this material. Although the phenomena of living beings cannot be accurately analyzed, yet the two antagonistic forces are the organic typal, or modal force, creative and preservative of the organic form; and the chemical forces of the material molecules, that keep the substances of the forms in endless change. To take for illustration the phenomena of cell-growth. Mechanical force governs the position, shape and relations of the cell; chemical force governs its composition; but vital force assimilates it to the organ of which it is to form part, and gives it the power to partake actively of the vital processes.

It would seem, if further argument were needed, that the simple fact of the abrupt cessation of the chemist's power of synthesis, at the verge of organic life, were enough to prove that vitality was distinct from physical laws.

Although it is true that science has gradually brought some phenomena of digestion, heat and nutrition under the dominion of chemical laws, yet the total failure to subject the nervous element; and its connection with mind, to any sufficient physical or material hypothesis, would also incline us to believe that there is indeed a higher law of life, inscrutable, omnipotent and omnipresent in the human organism, and that that power is vital force.

Such being the sum of our possible acquaintance with *vital force*, it is obvious that we have present in all our formulae of vital phenomena, in health or disease, an UNKNOWN ELEMENT, which no algebra or calculation can resolve, and which must at once vitiate the results of the numerical method.

But were all these fallacies of the object explained, or done away with, we should still have the liability to serious errors in the subject, or the observer himself. And first and most important of these is the influence of his own mind (the *ego*, as metaphysicians call it) upon the results of his observations.

The power of correct observation is innate: hence all observers

are not equally good. The facts, then, of various observations will not be alike, nor alike reliable: but there can be no just comparison of dissimilar or doubtful facts. We never separate the object from the perception; it is impossible to do so. What we call objects are, then, our ideas of them: and our ideas will vary according to our preconceived notions, and our judgment. These observations are not comparable. Probably it is through our judgment that we err oftenest. We are too credulous of notions which agree with our own, and too skeptical of the contrary. Everything that is novel irresistibly attracts some persons. In medical science we are ever ready, like children, to follow any authority.

When we express our own explanation of what we have observed, compared and grouped with many other things, we describe it as it appears to us. For this reason our whole previous knowledge and experience, our notions and views at the moment, acquire a much greater influence over the mass of our observations, at the bed-side, than we are conscious of. We are continually liable, in confounding our own notions with our observations, to fall into the same error as metaphysicians, who have been unable to solve their own problems, because the mind observing was the mind observed; the mental faculties were both the instrument and the object of investigation. These philosophers were obliged to employ a fallible insight to detect fallacies in their own minds.

It is plain that our tables of cases are filled with the vagaries of as many minds as there are observers. We cannot guard against their mistakes. Everything that suits us, assumes such a magnitude in our eyes, that it causes us to overlook many other important details. A deficient experience and youth formerly inclined us more to theorize from our observations. Maturity is a safe age for the observer and collector of empirical facts. But, at any age, we are too apt to make rash generalizations. The very talents of an individual with any peculiar tendency to explore only certain subjects, become injurious to the medical profession by establishing premature and special, or limited, theories.

[To be continued.]

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**PASSAGES FROM A SURGICAL NOTE-BOOK.—No. II.**

BY C. POWERS, M.D., MORAVIA, N. Y.

[Communicated for the Boston Medical and Surgical Journal.]

*Senile Gangrene in a Patient aged 93—Amputation—Recovery.*  
On the 9th of July, 1853, I was called upon to attend Thomas Atwood, who, the messenger said, was suffering from terribly acute pain in one of his feet.

Mr. Atwood was a farmer in comfortable circumstances, and

good habits, and his case was remarkable for the very unusual retention of his physical and mental powers, at an advanced age. At 93 he appeared scarcely 70, and he performed as much hard work as any of his laborers, asking none of his hired men to do more, or harder labor than himself. He often said that he was not conscious of any failure of his strength or capacity of endurance for the last thirty years, and supposed that Death must have forgotten him.

I found him suffering from agonizing pain in his right foot, which he said had come on, while he was riding in a waggon, as suddenly as though caused by a shot from a rifle. He was in good health, and had never, previously to this, had any similar attack. The foot appeared natural in color, but was somewhat colder than its fellow. There was also a marked sensation of numbness, and firm pressure on the part could hardly be felt by him. The foot and limb were placed in hot water in which was thrown plenty of mustard, and friction was kept up assiduously for two hours or more. To relieve the intense pain, a large dose of morphia was given, which had to be repeated again and again, before comparative relief was obtained.

On the next day, there was a still greater diminution of the temperature of the foot, notwithstanding all available means had been used to keep it up, and the whole foot and ankle had assumed a faint purple tinge. He could not now feel the point of a pin at any part below the ankle when thrust into, or even through the skin, but the pain was as excruciating as ever, when not quelled by enormous doses of morphia.

On the third day, the whole foot had a decidedly livid hue, and the faint purplish tinge was creeping up the limb. The severe pain had abated (and in a day or two more ceased entirely); tongue only slightly furred, appetite moderate, pulse nearly natural; not much constitutional disturbance, as yet. The foot and leg, nearly to the knee, were wrapped in a yeast and charcoal poultice, and beef and beef-tea were given freely, and brandy also, two ounces per diem, with diminished doses of morphia.

It is hardly necessary to give the minutiae of symptoms and treatment as the case progressed. Each day the dusky line advanced slowly upwards for about two weeks, when it paused at about three inches below the knee, though for the space of another fortnight there was no appearance of any line of demarcation. The foot and ankle had become coal-black, gangrenous, and blistered, the cuticle in many places detached, and the odor arising from the putrid limb exceedingly offensive. About a month from the first attack, nearly all of the calf of the leg, having partly sloughed, was removed, leaving the bone bare. All the dead portion was moist and putrid, not dry, or shrivelled. In the meanwhile brandy, quinine, porter, and beef-tea, were given in

large doses and quantities, to keep up the system, and enable it to make an effort to throw off the dead mass. The red line of demarcation was watched for with intense anxiety, but for more than a month ineffectually.

Before its appearance, should amputation have been performed? Authorities differ, and probably always will, on this point, though, unquestionably, the majority of good surgeons say no. Aside from the danger of the shock, in the low condition of the system, which the very nature of the malady implies, while the disease was progressing there was almost a certainty that gangrene would have attacked the stump. Bear in mind, also, that the age of the patient closely verged upon a century.

Once, about the third week, he came very near sinking, from what I supposed to be the effect of the absorption of morbid matter, from the gangrenous limb. He suddenly lost his appetite, his pulse ran up to 130, and with this, came delirium, dark and dry tongue, and tympanitic distension of the abdomen. At this stage, his symptoms singularly resembled those of advanced typhoid fever. The brandy, quinine, and essence of beef were increased, and in a few days these untoward phenomena all disappeared.

During the fifth week, the line of separation between the dead and living tissues became visible in places, and in a few days more completely encircled the limb. On the forty-second day, I removed the mortified portion, closely following the very irregular line of demarcation, separating it, most of the way, with the handle of the scalpel. The flap was turned back, and the bones divided about three inches below the knee, where they appeared healthy. There was scarcely any haemorrhage; no vessels requiring ligatures. The ragged flaps were then brought into apposition in such a way as to make as comely a stump as possible, fastened by adhesive straps, and supported so as to take off the pressure as much as practicable from the anterior corners of the remaining portions of the tibia and fibula. Ulceration of the thin integument, however, occurred at this point, and the ends of the bones appeared protruding through the skin; and whilst waiting a few days for Mr. A. to get a little more strength to bear a re-amputation of the bones, nature took the case into her own hands; and as she had already, nearly unaided, thrown off the dead soft parts, she now proceeded in her own way to remove the sequestrum. One day, while examining the projecting ends, I found they were loose and detached, and removed them with the dressing forceps. At the line of junction of the dead and living bone, they had been *girdled* by the action of the absorbents, looking very much as if they had been gnawed off by mice. There is not a particle of doubt, that, if I had waited a little longer, there would have been a complete spontaneous amputation of the whole limb, and the patient would probably have made as good recovery as he did.

The healing process then went on favorably, and in a few months.

he was well—with the exception of the mutilation—having a good stump. He died the next year, of congestion of the lungs.

*Moravia, N. Y., January, 1861.*

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## THE BOSTON MEDICAL AND SURGICAL JOURNAL.

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BOSTON: THURSDAY, JANUARY 17, 1861.

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MASSACHUSETTS MEDICAL SOCIETY.—We have received a copy of the revised edition of the By-Laws of the Massachusetts Medical Society, which has just been issued in accordance with a vote of the Councilors, and containing all the amendments which were made at the Annual Meeting of the Society in May, 1860. It bears the marks of much care in revision, for which the Society is largely indebted to the judgment and discrimination of the Recording Secretary.

The attention of the Fellows is particularly called to the change in the *mode* of admission. Formerly any graduate of the Harvard or Berkshire Medical Colleges could be admitted by the Recording Secretary, or the Secretary of any District Society, upon presentation of his medical degree, and signing the By-Laws. This power has been annulled by the alteration alluded to, and at present *no person* can be admitted a Fellow except upon examination by a Board of Censors. The only reservation in favor of the Harvard and Berkshire Medical Institutions, is, that “graduates from those Colleges shall, if otherwise qualified, be admitted without further examination of their medical attainments.” This change makes it important that Secretaries of Boards of Censors should especially note the provisions contained in the 15th and 22d Sections, in order that the Recording Secretary may be able to furnish the Society with a complete list of new members at its Annual Meeting.

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It gives us great pleasure to inform our readers that the contribution in the JOURNAL to-day, from Dr. JOHN WARE, on General Therapeutics, is only the first of a series of articles upon this very important subject. For their introduction, no more appropriate time could have been chosen than the present, when the minds of many appear to be unsettled in regard to the value of therapeutic agents. We are sure that all will be gratified to hear from one, to whom high position, experience and wisdom give the best right to speak.

The views expressed in the following extract, from the *Columbus (Ohio) Review*, for December, 1860, are so just, and so accordant with our own, that we take the liberty of copying them. The writer, in noticing the recent paper by Dr. Ware on Haemoptysis, says:—

“In medical as in all literature there are scores of authors who have written well but too much; the gold they give us is too much beaten, and, however well adapted for certain ornamental purposes, does not serve, like compact coin, to buy meat and raiment. Time and change make sad rents into such tissues. Others have added treasures to our stores of medical thoughts—have even investigated, elaborated and classified the various phenomena of disease and health, but have done it in a most execrable manner, and to get at anything worth having we have to encounter much that is unpleasant. Others bring facts to the

general store as cartmen bring paving boulders in a cart and dump them on to the highway in a heap, from which you may pick and choose (if you can get at them) any that suit your purpose, where they lay. Dr. Ware belongs to neither of these classes. What he has done he has done well. He has added to our stores of knowledge upon medical subjects, and presented his materials in so quiet and proper a guise, that they have passed at once and irrevocably into their proper place in the ranks. To use a simple expression, every hit he has made has *told*. But he has done so little to enrich our literature, that every little work he has given us has only tended to deepen our regret that so cautious an observer, so patient a generalizer, so careful a theorist, and so judicious a practitioner, has not left us a more enlarged work upon medical science.

"But however much we may regret this, we recollect with pleasure that his short monographs on Croup, on Delirium Tremens, and his occasional contributions to medical literature, have been incorporated into the large mass of standard authority in medicine. It is therefore a great source of satisfaction to greet another work (though only a pamphlet) from his pen. The modest pamphlet before us contains more real matter than many a bulky volume we could mention."

MORTALITY STATISTICS OF PROVIDENCE, R. I.—In Providence the increase of deaths in 1860 over those of 1859 was 102, or 11.3 per cent. Of the whole increase, there were 34 during the first, and 68 during the second six months of the year. The *increase* of deaths in 1860 from several important diseases was as follows: cholera infantum, 35; consumption, 30; diphtheria, 8; diseases of the heart, 8; unknown causes, 11. The *decrease* from several diseases was as follows: croup, 6; diarrhoea, 8; fevers, 17; malformations, 8; old age, 7; scarlatina, 28.—*Providence Daily Journal*.

DIPHTHERITIS.—Dr. J. W. Smith, of Wellington, Ohio, says, in the *Cincinnati Lancet and Observer*, "This disease is now prevailing to an alarming extent in many neighborhoods of Northern Ohio, assuming, too often, a character so formidable as to challenge the best thoughts of the best men in our profession. . . . In a country practice of not immoderate extent, I have been called to treat probably one hundred and fifty cases within the last four months. All these have been well marked, though the majority readily recovered."

AN INDIAN MEDICAL JOURNAL has been established in Oordoo, E. I., entitled the *Akbare Tubabut*. It is intended as a medium of communication between native doctors in government employ and native Hakkims, for the improvement of medical and surgical knowledge, and the greater alleviation of the many diseases to which the millions of inhabitants of this country are subject.—*Am. Jour. of Dental Science*.

CHLOROFORM IN ITCH.—The *Druggists' Circular* says: "Prof Bock has found the external application of chloroform useful in some cases of itch. This substance appears to kill the insect, and, moreover, by producing anaesthesia, it relieves the irritability of the skin. M. Bock has never observed any inconvenience to arise from the use of chloroform, and the sensation of burning, which it produces for a short time, is quite trifling in comparison with the intolerable itching caused by the disease."

At the Annual Meeting of the New York Medical and Surgical Society, held on the 5th inst., Dr. Alonzo Clark was elected President; Dr. T. M. Halstead, Vice President; Dr. H. D. Sands, Secretary; and Dr. Thomas F. Cock, Treasurer.—*Am. Medical Times*.

**WANT OF HONESTY IN MAKING PHARMACEUTICAL PREPARATIONS.**—M. L. Leroy, of New York, has sent us a piece of thick porous blotting paper, laid off in squares with a pencil; each square contained a stain made by a drop of some pharmaceutical liquid—each column consisted of different specimens of the same preparation, and exhibits, at a glance, the variation of composition by the difference in the stains. He considers this a good way to test for many of those preparations that have an ingredient that is liable to vary from its high price—as, for instance, saffron.—*Am. Journal of Pharmacy.*

Dr. R. W. GIBBS has been appointed, by the Governor of South Carolina, Surgeon-General of that State; and Drs. F. P. Porcher and J. J. Chisolm, Surgeons to the U. S. Marine Hospital, now held by South Carolina.—The New Orleans *Med. News and Hospital Gazette* states, that “ notwithstanding the terrible financial crisis and unprecedented political excitement which exists,” the class of the School of Medicine of that city is larger than usual.—Students in Paris, to the number of 1196, have registered at the Faculty of Medicine.

**VITAL STATISTICS OF BOSTON.**  
FOR THE WEEK ENDING SATURDAY, JANUARY 12th, 1861.  
DEATHS.

	Males.	Females.	Total.
Deaths during the week,	35	38	73
Average Mortality of the corresponding weeks of the ten years, 1850-1860,	37.6	37.0	74.6
Average corrected to increased population,	..	..	83.3
Deaths of persons above 90,	..	..	..

Mortality from Prevailing Diseases.							
Phthisis.	Croup.	Scar. Fev.	Pneumonia.	Measles.	Smallpox.	Dysentery.	Typhoid Fever.
15	5	5	6	0	0	0	0

METEOROLOGY.									
From Observations taken by Dr. Ignatius Langer, at Davenport, Scott Co., Iowa.									
North.      Latitude, 41.31 Longitude, 134.41 West.      Height above the Sea, 585.									
BAROMETER.									
7 A.M.	2 P.M.	9 P.M.							
Monday, Dec. 31,	29.92	29.81	29.29						
Tuesday, Jan. 1,	29.69	29.63	29.58						
Wednesday, " 2,	29.55	29.69	29.61						
Thursday, " 3,	29.63	23.58	29.60						
Friday, " 4,	29.63	29.61	29.61						
Saturday, " 5,	29.62	29.62	29.56						
Sunday, " 6,	29.34	29.17	29.11						
THERMOMETER.									
7 A.M.	10	18	20						
2 P.M.	13	33	27						
9 P.M.	19	28	21						
Mean Point,	10	16	12						
Highest Point,	29.11								
Lowest Point,									
Mean Height,	17.56								
SNOW.									
Time	2 hours,								
Mean	0.22								
Amount									
Cloud,									
0 to 10.									

NOTICE.—We are requested to state that the revised edition of the By-Laws of the Massachusetts Medical Society, just published, was mailed from this office, to all the active members of the Society, on the 14th inst.

COMMUNICATIONS RECEIVED.—Case of Nasal Polypus.

BOOKS RECEIVED.—Contributions to the Anatomy of the Spinal Cord. By Dr. J. F. Trask, of San Francisco.—Account of a New Cranial Perforator. By T. Gaillard Thomas, M.D., New York.—Twenty-Second Annual Report of the Board of Trustees and Officers of the Central Ohio Lunatic Asylum.

DIED.—At Mount Pleasant, Iowa, Nov. 8th, Dr. N. Bruce, aged 72 years.

DEATHS IN BOSTON for the week ending Saturday noon, January 12th, 1861, 73. Males, 35—Females, 38.—Accidents, 2—apoplexy, 1—disease of the bowels, 2—inflammation of the bowels, 1—bronchitis, 4—congestion of the brain, 1—disease of the brain, 2—disease of the heart, 2—cancer, 2—consumption, 15—convulsions, 1—croup, 5—debility, 1—dropsy of the brain, 4—epilepsy, 1—erysipelas, 3—scarlet fever, 5—disease of the heart, 2—intemperance, 1—flu, 1—inflammation of the lungs, 6—marasmus, 1—old age, 1—paralysis, 1—pleurisy, 1—premature birth, 3—phlebitis, 1—teething, 1—unknown, 2.

Under 5 years of age, 28—between 5 and 20 years, 22—between 20 and 40 years, 21—between 40 and 60 years, 9—above 60 years, 13. Born in the United States, 49—Ireland, 10—other places, 5.